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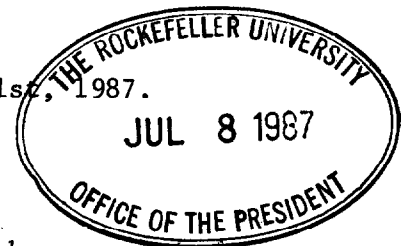
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LABORATOIRE DE CYTOLOGIE ET
EMBRYOLOGIE MOLECULAIRES

J. BRACHET, Prof. Emérite

A rappeler dans la réponse :

Brussels, July 1st, 1987.



Dr. Joshua Lederberg,
The Rockefeller University,
1230 York Avenue,
New York, N.Y. 10021.

Dear Professor Lederberg,

It was a great pleasure to find your kind and interesting letter when I came back to the lab after we spent a few weeks in the South of France. I am very happy that my reminiscences about RNA cytochemistry awoke in you a "pleasant nostalgia". In fact, I did not know that you had been personally involved in the nucleolar RNA enigma. This is no longer a surprise for me now that I know that my old friends Mirsky and Pollister had an influence on your first steps in research.

It is true that my note in C.R. Soc. Biol. 133, 88, 1940 is the first I published on RNA cytochemistry. However, I was already puzzled by the biochemical significance of pyronine staining in 1938-1939. I was working with Louis Rapkine and I remember we had a bet (a glass of beer) : is basophilia due to -SH groups or to RNA ? When Rapkine came back to Brussels, I had the answer : staining is abolished by pretreatment with ribonuclease, not by pretreatment with mono-iodacetate. This was in 1939, since Louis had to stop his visits to Brussels as soon as France declared war on Germany (Sept. 3, 1939).

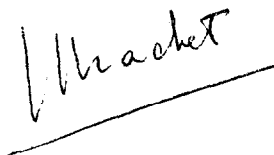
Regarding the work of Caspersson, you are right : his evidence for the presence of RNA in the nucleolus was based on UV-absorption and Feulgen negativity. For reasons of his own, he always refused to use ribonuclease as a cytochemical tool. Of course there was a kind of competition between Stockholm and Brussels in those days and occupied Belgium was at a disadvantage to neutral Sweden in the matter of publications (it was almost treason for us to publish in a German scientific journal). The best place for finding an historical sketch of this period is, I believe, my old book "Embryologie chimique", 1944, 1945 (translated in English by L.G. Barth in 1950). In addition to the

articles for The Scientist, I recently wrote a review of my old work in International Review of Cytology (Vol. 100) and an article for TIBS on "Cytochemistry and Biochemistry". But they do not add much to what you found in "The Scientist".

I have read with great interest the articles about yourself you kindly sent me. I have, like everybody, the utmost admiration for your work on genetic recombination in bacteria : this was a major contribution of Genetics to Microbiology. I remember very well meeting you some 40 years ago at an ESH Symposium.

With many thanks for your kind letter.

Yours sincerely,

A handwritten signature in cursive script that reads "J. Brachet". The signature is written in dark ink and is positioned above a single horizontal line that serves as a separator.

J. Brachet.